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PLACE-BASED POLICIES AND THE HOUSING MARKET

Hans R. A. Koster and Jos van Ommeren*

Abstract—We study the economic effects of place-based policies in the housing market, by investigating the effects of a place-based program on prices of surrounding owner-occupied properties. The program improved the quality of public housing in 83 impoverished neighborhoods throughout the Netherlands. We combine a first-difference approach with a fuzzy regression-discontinuity design to address the fundamental issue that these neighborhoods are endogenously treated. Improvements in public housing induced surrounding housing prices to increase by 3.5%. The program's external benefits are sizable and at least half of the value of investments in public housing.

I. Introduction

IN many countries, place-based policies have been developed that make large public investments in poor neighborhoods. Economists are not necessarily in favor of these policies. It has been argued that governments should help people rather than places, and “not bribe people to live in unattractive places” (Glaeser, 2011). However, if nonmarket interactions are important, this may justify place-based policies. In Europe, place-based policies often improve the quality of the public housing stock through new home construction to replace an obsolete building stock or through substantial renovations to the existing stock.¹ This benefits not only public housing tenants but also nearby residents through higher neighborhood quality.

Ample attention has been paid to the effectiveness of place-based labor market programs (Neumark & Kolko, 2010; Mayer, Mayneris, & Py, 2012; Busso, Gregory, & Kline, 2013; Kline & Moretti, 2013; and Neumark & Simpson, 2015, for an overview). However, the effects of place-based housing policies on local residents have not been researched much. Few studies confirm that place-based investments have led to higher house prices (Santiago, Galster, & Tatian, 2001; Schwartz et al., 2006; Rossi-Hansberg, Sarte, & Owens, 2010). This does not imply, however, that place-based policies are always effective. For example, a number of studies, including De Souza Briggs (1999), Lee, Culhane, and

Wachter (1999), and Ahlfeldt, Maennig, and Richter (2016), find no statistically significant, or even small negative effects of place-based policies that subsidize housing.

While programs to upgrade public housing are common in many cities (e.g., in Australia, France, Spain, United Kingdom, and the United States), settings where it is feasible to credibly identify spillover effects from these large-scale housing investments are uncommon. Typically, these studies focus on a specific program with a small number of neighborhoods in a specific city. Furthermore, because neighborhood selection is endogenous—only the worst-performing sites receive subsidies—the estimates of the benefits of the program may not be causal.

We evaluate the effects of an unusually large, nationwide urban revitalization program in the Netherlands, starting in 2007, which sought to improve the quality of public housing. We aim to measure external effects by focusing on changes in prices of owner-occupied housing units, which were not improved by the program. In this so-called *krachtwijken*-program (henceforth, KW-investment scheme), 83 neighborhoods were selected for revitalization with funding from the national government.² The government and (nonprofit) public housing associations announced investing about €2.75 billion in these neighborhoods, on average about €7,000 per household in receiving neighborhoods, but eventually only €1 billion was spent (Permentier, Kullberg, & Van Noije, 2013). The main objectives of the program were to transform these neighborhoods into pleasant places to live and to reduce social inequality (Department of Housing, Spatial Planning and the Environment, 2007). In the end almost all of the money (90%) was spent on improving the quality of the public housing stock. The remainder was spent on green spaces and social empowerment programs (Wittebrood & Permentier, 2011). We use a nationwide data set with information on thousands of privately owned repeat-sales observations from 2000 to 2014.³

The main contribution of this paper is the identification of causal effects of place-based policies on property values. We take into account that neighborhoods targeted by place-based policies are not randomly chosen but are explicitly chosen because of undesirable characteristics. We combine a first-differences estimation strategy with a regression-discontinuity design by using information on an eligibility criterion to receive investments. Hence, we compare the change in housing prices close to the z-score threshold. This criterion is dependent on deprivation scores,

²The scheme was also known as *aandachtswijken*-scheme or *Vogelaarwijken*-scheme.

³About 90% of the Dutch rental housing stock is rent controlled, and about 60% of stock is owner occupied. We do not expect to detect any effect on the controlled public housing rent, but there may be effects on rents of private rental housing. Because of lower data quality for private and public housing rents, we examine this in the sensitivity analysis (see appendix C.4).

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¹In Europe, public housing is common and covers 47% of the rental market (Van Ommeren & Van der Vlist, 2016).

calculated by the national government. However, there are fourteen noncomplying neighborhoods with scores that were too low but were selected or had sufficiently high scores but did not receive treatment in the end. We therefore use a fuzzy regression-discontinuity design (FRD), for which it was necessary to observe a substantial jump in the probability of being treated. Indeed, at the neighborhood level, we observe a more than 90% increase in the probability of being treated when the deprivation score exceeds a certain threshold. Moreover, we show that there is no bunching at the threshold confirming that z -scores could not be influenced by local governments.

We generalize the results into two directions. First, we make a distinction between the short-run and long-run effects of place-based policies. This distinction is relevant, because we are mainly interested in the extent to which beneficial place-based policies increase house prices in the long run. These policies are thought to reduce sales times temporarily (i.e., in the short run) but do not affect sales time in the long run, while prices should adjust almost immediately when new information becomes available.⁴ Estimates of the temporal policy effects on sales time and prices are then indicative how much time it takes before the market returns to a long-run steady state and can be used as an internal consistency test: if one does not find a temporary effect of place-based policies on sales times while finding a permanent effect on prices, then this will put doubt on the causality of an effect on prices.⁵

Second, we pay attention to treatment heterogeneity by investigating whether treatment is more effective in more deprived neighborhoods and by including interactions of neighborhood demographics, such as population density and neighborhood income, with the treatment effect.⁶

We find that due to place-based investments that improve the quality of public housing, house prices increased by at least 3.5%. Sales times are reduced temporarily and bounce back to their initial levels in about 7.5 years. We also find that the effect is much stronger in dense areas. For example, when population density doubles, the treatment effect is 4.3 percentage points higher. This is likely explained by spillovers that are more pronounced when properties are closer to each other. We further find that neighborhoods that

have received more funding per square meter also have experienced stronger price increases. For example, the treatment effect for the average spending is 2.1%, while this can easily triple for neighborhoods for which the treatment intensity is higher. Eventually, we calculate that the welfare benefits to property owners induced by the program are at least half of the value of the investments in public housing. Moreover, we show that renters in public housing have not faced a rent increase yet have seen an increase in neighborhood quality. Hence, they are definitely better off, in particular because they may also have benefited from direct improvements in their properties. In other words, the program seems to have been effective in increasing the utility of poorer households. An extensive sensitivity analysis confirms these results.

The remainder of the paper is organized as follows. In section II, we discuss the features of the KW-investment scheme and the selection of the neighborhoods. Section III elaborates on the econometric framework, the data, and some graphical descriptive evidence. Section IV turns to the empirical results, followed by a summary of sensitivity analyses in section V. Section VI is a calculation of the overall gains of the program and section VII concludes.

II. Local Context

A. The Urban Revitalization Program

There is ample empirical evidence that households with low incomes and associated social problems are disproportionately concentrated in certain urban neighborhoods. For example, many U.S. inner cities contain large concentrations of low-income households and score high on most measures of social dysfunction (Mills & Lubuele, 1997; Glaeser, Kahn, & Rappaport, 2008; Rosenthal & Ross, 2015). In the Netherlands, we observe a similar but less extreme pattern due to the existence of substantial benefit transfers and the universal provision of public housing. About 70% of the most deprived neighborhoods are located in the four largest cities (Amsterdam, Rotterdam, the Hague, and Utrecht). The share of public housing is much higher in these neighborhoods than in other parts of the Netherlands. The gap between deprived and other neighborhoods in terms of unemployment, crime rates, and income has widened in the past decade. Therefore, in 2007, a substantial national investment program was launched by the secretary of state responsible for housing and labor: €216 million was planned to be invested in the 83 worst-performing postal code areas, which we refer to as neighborhoods (Court of Audit, 2010). The average size of a targeted neighborhood is 1.43 square kilometers, so neighborhoods are rather small. The investment fund was used to assist municipalities in restructuring and revitalizing neighborhoods. On September 14, 2007, the secretary of state agreed with large public housing associations that they would invest another €2.5 billion in the selected neighborhoods in public housing over a course of ten years (in total, about €7,000 per household

⁴See appendix A for a formal derivation of these results based on a stylized model extending the search and matching framework by Wheaton (1990). This model combines a standard spatial equilibrium framework with buyer search costs that are proportional to house prices in the long run.

⁵One conceptual difficulty is that our observations of sales times imply the presence of housing vacancies, so changes in house prices do not necessarily reflect changes in welfare. We demonstrate in appendix A that given the assumption of a spatial equilibrium, one may ignore this issue because changes in long-run prices induced by place-based policies are slight underestimates of welfare changes when the housing vacancy rate is low, which is usually the case.

⁶A recent study by Diamond and McQuade (2016) analyzes the effects of increases in the quantity of subsidized houses and finds heterogeneous effects due to the increase of low-income households in the area: housing subsidies cause house prices to decrease in higher-income areas while generating house price increases in lower-income areas, which is mainly due to sorting effects.

residing in these neighborhoods) (Court of Audit, 2010).⁷ Although the exact expenditure is unknown, experts estimate that in the end, about €1 billion was invested between 2007 and 2012 (Permentier et al., 2013). About 90% of the money was spent on reinvigorating public housing. Upgrading entails painting the exterior and upgrading the outside appearance of the buildings, adding double glazing and insulation, adjusting gardens belonging to apartment blocks, and sometimes demolishing deteriorated housing and replacing it with new apartments. After 2012, the program was abolished.

Arguably, the physical restructuring of public housing has a beneficial effect on nearby residents, who prefer to live in a well-maintained building environment (Rossi-Hansberg et al., 2010). Such an environment not only improves views, but also may improve physical and mental health, according to a large environmental psychology and health literature (Srinivasan, O'Fallon, & Dearry, 2003). Apart from physical restructuring and sale of public housing, a small share of the investment was targeted at poor households directly through empowerment programs (Department of Housing, Spatial Planning and the Environment, 2007; Wittebrood & Permentier, 2011).

Another effect of the program may be indirect. If the social composition of a neighborhood changes due to the program, this may have impacts on house prices. For example, empirical evidence suggests that high-income households are disproportionately attracted by amenities (Gagné, Koster, Moizeau, & Thisse, 2018). Furthermore, it may be that upgrading public housing will have a differential effect on high- and low-income neighborhoods, as Diamond and McQuade (2016) documented in their study on an increase in the quantity of subsidized housing. We will show that there are minor changes in the social composition in the treated neighborhood, but controlling for demographics, including neighborhood income, leaves the price effect essentially unaffected. Heterogeneity of the treatment effect related to the demographic composition is also minor. For example, we do not find any evidence that the effects of KW-investments depend on neighborhood income level. Hence, most of our effect seems to be explained by improvements in the physical appearance of neighborhoods.

B. Selection of Neighborhoods

To select eligible neighborhoods, we used so-called deprivation scores consisting of eighteen indicators. The indicators were organized in four categories: social deprivation (income levels, education, and unemployment), physical deprivation (quality of housing stock), social problems (vandalism and crime), and physical problems (noise and air pollution, satisfaction with living environment). It is important to note that our outcome variables (house prices, rents, sales times) were

not one of the indicators. Brouwer and Willems (2007) use data from 1998, 2002, and 2006 to calculate so-called deprivation z -scores for each postal code area in the Netherlands with at least 1,000 inhabitants (about 4,000 areas), where each of the four categories is weighted equally and standardized with mean zero and unit standard deviation. Because the overall z -score is the sum of the standardized scores of four categories, the average score is 0, but the standard deviation of the overall z -score exceeds 1.

The selection of the KW-neighborhoods was based on the deprivation score. Only neighborhoods are considered that have a lower-than-average z -score for each category (hence, a z -score for each of the categories lower than 0) were considered (Permentier et al., 2013). Furthermore, neighborhoods with a z -score of at least 7.30 were eligible. However, four neighborhoods were removed from the list because they did not have a lower-than-average z -score on each of the categories. Eight other neighborhoods, after discussion with local governments, were also removed. These were mainly downtown neighborhoods for which the recorded nuisance was related to retail, nightlife, and entertainment activities, which are not characteristics of deprivation. In addition, the local governments of Amsterdam and Enschede argued that two neighborhoods in their locality for which the z -score was sufficiently high (above 7.3) should be replaced by two neighborhoods that were below the z -score (respectively, 6.84 and 5.00) because the latter neighborhoods were argued to be experiencing more deprivation.⁸ In the end, this implies that fourteen neighborhoods did not comply with the scoring rule. More information on the selection procedure and the noncomplying neighborhoods is listed in appendix B.1.

Table 1 reports the z -scores for each of the categories. Unsurprisingly, targeted KW-neighborhoods have scores that are much higher than the Dutch average for each of the categories.

In our empirical analysis, we exploit exogenous variation using the arbitrary threshold of 7.3 to identify the causal effect of the program. We illustrate some of the features of our research design and test some assumptions underlying the regression-discontinuity design we employ later. We start the analysis by plotting the assignment as a function of z -scores in figure 1A. While controlling flexibly for the z -score on both sides of the boundary, we show a substantial discrete jump in the probability of being selected when $z \geq 7.30$. For example, a neighborhood with a z -score of 7.29 has a probability of 2.4% of being included, whereas for a neighborhood with a z -score of 7.30, this probability is 78%. In the empirical analysis, we exclude observations within 2.5 kilometers of a treated neighborhood. Because many noncomplying neighborhoods are relatively close to treated neighborhoods, the jump will then increase to more than 90%.

⁸There was substantial criticism on the selection of the specific neighborhoods. According to opponents, the selection criteria were not well chosen and the postal code areas were too large to capture meaningful neighborhoods. In contrast, we think that neighborhoods are fairly small; the average distance to the centroid of a neighborhood is only 286 meters.

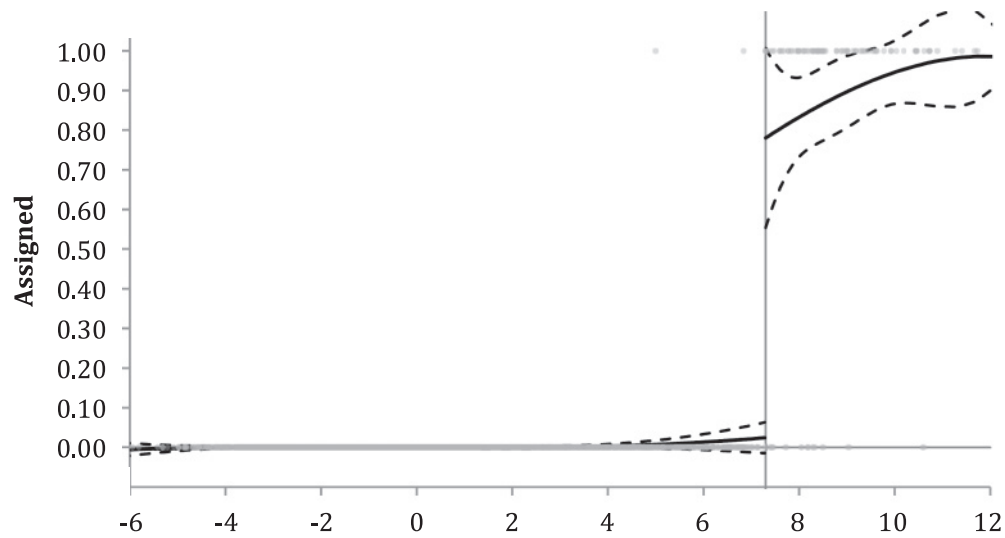
⁷We consider September 14, 2007, as the start of the investment program, but we will check for robustness of the assumed date later.

TABLE 1.—DEPRIVATION SCORES FOR NEIGHBORHOODS

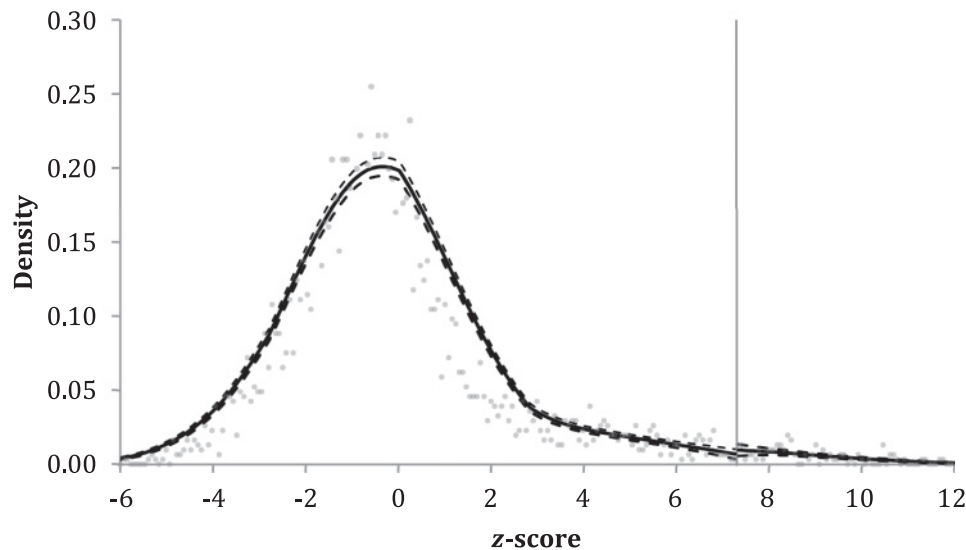
	All Neighborhoods		KW-Neighborhoods		Non-KW-Neighborhoods	
	μ	σ	μ	σ	μ	σ
Social deprivation	0.00	0.654	1.167	0.322	−0.0246	0.636
Physical deprivation	0.00	0.611	2.070	0.660	−0.0437	0.529
Social problems	0.00	0.924	2.612	1.053	−0.0551	0.838
Physical problems	0.00	0.950	3.087	0.976	−0.0651	0.834
Overall	0.00	2.414	8.935	1.340	−0.188	2.047
Number of neighborhoods	4,016		83		3,933	

Social deprivation has three indicators: income, unemployment, and low education share. Physical deprivation has three housing quality indicators: shares of small houses, old houses (constructed before 1970), and public housing stock. Social problems consists of five indicators: two vandalism indicators, two nuisance-from-neighbors indicators, and one indicator relating to feelings of insecurity. Physical problems has seven indicators: house and living environment satisfaction, the inclination to move, and indicators relating to noise and air pollution, traffic intensity and traffic safety. For details, see Brouwer and Willems (2007).

FIGURE 1.—THE Z-SCORE



(A) The Z-Score and Selection



(B) Manipulation Test for Z-Scores

(A) A regression of the assignment of a neighborhood on the scoring rule dummy and a third-order polynomial of the z -score on both sides of the boundary. (B) We estimate the test developed by McCrary (2008) to investigate whether the running variable (the z -score) is continuous around the threshold. The dotted lines represent 95% confidence intervals.

An important assumption of a RDD is that the density of the z -score is continuous at the threshold. Otherwise, neighborhoods may have manipulated the z -score and therefore the propensity to become treated. The latter is implausible because the deprivation score was a function of eighteen indicators that are very difficult to influence in the short run (including subjective feelings about the neighborhood, level of education of residents, and housing stock). What is more important, the investment program was announced in 2007, based on data from 2006, 2002, and 1998. It is therefore highly unlikely that local governments anticipated the exact selection criteria. More formally, we estimate a McCrary (2008) test for bunching around the threshold. This test investigates whether the density of the z -score is continuous at the threshold. Figure 1B shows that this is indeed the case, which supports our claim that local governments could not manipulate the z -scores.

III. Empirical Framework, Data, and Graphical Analysis

A. A Regression-Discontinuity Design

We are interested in the causal effect of the KW-investment scheme on surrounding house prices. Let $\log p_{\ell t}$ be the logarithm of the house price per square meter for a housing unit in neighborhood ℓ in year t . The house price is a function of whether the neighborhood has received investments $k_{\ell t}$ in year t .

When estimating the causal effect of $k_{\ell t}$ on prices, one faces three main issues. The first is that spatial spillovers of the KW-program may exist: houses close to a targeted area may also experience changes in $p_{\ell t}$ due to investments in adjacent neighborhoods (see Rossi-Hansberg et al., 2010). Not controlling for spatial spillovers may lead to a strong underestimate of the program's benefits. In the preferred specifications, we therefore exclude observations within 2.5 kilometers of a targeted neighborhood. In a sensitivity analysis (see appendix C.7), we investigate the presence of potential spatial spillovers.

The second issue is that the treatment is explicitly non-random: the most deprived neighborhoods are targeted. To resolve this issue, we employ a first-differences approach, where the change in the price, $\Delta p_{\ell t}$, is regressed on the change in the investment. By construction, $\Delta k_{\ell t}$ then equals 1 when we observe a property located in a targeted area before and after the starting date of the program and equals 0 otherwise. Each observation of changes in prices refers to two housing transactions. Because we have an unbalanced panel, only a certain percentage of the observations in treated neighborhoods refers to transactions before and after the treatment. In the empirical analysis, the preferred specifications therefore include only observations that refer to changes before and after the starting date of the program. To further control for changes to the house (e.g., improvements in maintenance that may disproportionately occur in neighborhoods

with older houses), we include changes in housing variables $\Delta x_{\ell t}$.

The third issue is that while first-differencing may control for all time-invariant differences between neighborhoods before treatment, it does not address the issue that unobserved trends may be correlated with the change in treatment, $\Delta k_{\ell t}$. This may be problematic when demographic trends such as gentrification are correlated to the probability of being treated. To address this issue, we need to find neighborhoods that are almost identical to KW-neighborhoods but are not targeted by the investment scheme.

An identification strategy that comes close to random sampling is a regression-discontinuity design (RDD). In this paper, we combine first-differencing with an RDD based on the deprivation score of the neighborhood. This implies that we compare changes in prices close to the z -score threshold. This approach approximately provides the causal effect of the investment if neighborhoods are not able to manipulate the score. We already argued and showed in the previous section that it is extremely unlikely that manipulation is a problem.

Although local governments could not manipulate the neighborhood score, some neighborhoods were removed from the ultimate list and replaced by others after discussions with the local governments (as discussed in the previous section). This makes a standard sharp regression-discontinuity design (SRD) invalid, as it assumes a one-to-one relationship between the assignment and the z -score. We then employ a fuzzy regression-discontinuity design (FRD) because the neighborhoods that were removed may be a nonrandom selection of eligible neighborhoods. An FRD can be interpreted as an instrumental variables approach (Imbens & Lemieux, 2008).

In principle, to avoid any bias, one would prefer to include only observations that are at the z -score threshold, so $c = 7.30$. However, this would lead to few observations and therefore to large standard errors. Hence, we use a local linear (LL) regression approach, where observations close to the threshold receive a higher weight (Hahn, Todd, & Van der Klaauw, 2001).

In the first stage, we regress the change in investment status on a dummy whether the neighborhood was eligible based on the scoring rule and timing:

$$\left(\hat{\pi}, \hat{\beta}, \hat{v}_t \right) = \arg \min_{\tilde{\pi}, \tilde{\beta}, \tilde{v}_t} \sum_{i=1}^N K \left(\frac{z_{i\ell} - c}{h} \right) \times \left(\Delta k_{\ell t} - \tilde{\pi} \Delta s_{\ell t} - \tilde{\beta} \Delta x_{\ell t} - \tilde{v}_t \right)^2, \quad (1)$$

where the \sim indicates first-stage coefficients, $\tilde{\pi}$ is the parameter of interest, and $K(\cdot)$ denotes the kernel function.

Note that $\Delta s_{\ell t}$ equals 1 when $z \geq 7.30$ and when a property is sold before and after the investment. In the second stage, we insert $\hat{\Delta k}_{\ell t}$ (and calculate standard errors taking into account

that $\Delta \hat{k}_{\ell t}$ is estimated):

$$(\hat{\alpha}, \hat{\beta}, \hat{v}_t) = \arg \min_{\alpha, \beta, v_t} \sum_{i=1}^N K \left(\frac{z_{i\ell} - c}{h} \right) \times (\Delta \log p_{\ell t} - \alpha \Delta \hat{k}_{\ell t} - \beta \Delta x_{\ell t} - \Delta v_t)^2. \quad (2)$$

Throughout the analysis, we adopt a uniform kernel:

$$K \left(\frac{z_{i\ell} - c}{h} \right) = 1_{|z_{i\ell} - c| < h}, \quad (3)$$

where h is the bandwidth that determines how many observations are included on both sides of the threshold. The estimated parameters are usually sensitive to the choice of the bandwidth. We use the approach proposed by Imbens and Kalyanaraman (2012) to determine the optimal bandwidth. Because we employ an FRD, the formula to determine the optimal bandwidth is somewhat modified, but note that the optimal bandwidth in a FRD is usually very similar to the optimal bandwidth in an SRD. (See appendix B.3 for the derivation of the optimal bandwidth.)

Note that a regression-discontinuity design identifies the local average treatment effect at the threshold. If treatment effects vary across targeted areas (e.g., a euro invested in the most deprived neighborhood is more effective than a euro invested in the eighty-third deprived neighborhood), the local average treatment effect would differ from the average treatment effect of the policy. Nevertheless, when α would be similar to the estimation procedure where we include all neighborhoods ($h \rightarrow \infty$), this would suggest that the local average treatment effect at the threshold is equal to the average treatment effect.

To get more insight into the mechanism of the effects, we also gather data on demographic variables of the neighborhood, such as average neighborhood income, population density, and share of foreigners. If the place-based investment mainly refers to an improved quality of the neighborhood, we expect that adding these variables will not change the coefficient of interest. This will add to the credibility of the regression-discontinuity design: in a valid RDD, adding control variables does not affect the consistency of the estimated parameter. But if sorting effects are very important, part of the positive effect of place-based policies might be explained by changes in the demographic composition of a neighborhood (Rossi-Hansberg et al., 2010; Diamond & McQuade, 2016).⁹

⁹In a standard hedonic regression, changes in neighborhood demographics are usually endogenous. However, because of our research design, this should not be the case, as changes in neighborhood demographics close to the threshold should be (almost) identical in the absence of the program. In appendix C.2, we instrument for potentially endogenous neighborhood characteristics with shift-share instruments and show that the results are then virtually identical to the results where we do not control for neighborhood demographics.

B. Adjustment Effects and Treatment Heterogeneity

We are also interested in adjustment effects after the investment has taken place. In appendix A, we outline a standard spatial equilibrium model that we combine with the search and matching framework of Wheaton (1990). When we assume that search costs are proportional to amenity levels with a delay, we show that sales time drops in the short run, while this effect disappears in the long run. The time it takes for sales time to adjust to the former value is indicative of the time for the housing market to return to a steady state. This helps to identify the long-run price effect. Let $d_{\ell t}$ be a variable that indicates how many years after the investment the transaction has taken place. We estimate

$$(\hat{\alpha}, \hat{\beta}, \hat{\delta}_p, \hat{v}_t) = \arg \min_{\alpha, \beta, \delta_p, v_t} \sum_{i=1}^N K \left(\frac{z_{i\ell} - c}{h} \right) \times \left(\Delta \log p_{\ell t} - \alpha \Delta k_{\ell t} - \sum_{p=1}^{\mathcal{P}} \delta_p \Delta(k_{\ell t} \times d_{\ell t})^p - \beta \Delta x_{\ell t} - \Delta v_t \right)^2, \quad (4)$$

where α indicates the immediate effect and δ_p are parameters that capture adjustment effects. We define $\log s_{\ell t}$ to be the logarithm of days on the market. We also estimate

$$(\hat{\zeta}, \hat{\eta}, \hat{\theta}_p, \hat{\varphi}_t) = \arg \min_{\zeta, \eta, \theta_p, \varphi_t} \sum_{i=1}^N K \left(\frac{z_{i\ell} - c}{h} \right) \times \left(\Delta \log s_{\ell t} - \zeta \Delta k_{\ell t} - \sum_{p=1}^{\mathcal{P}} \theta_p \Delta(k_{\ell t} \times d_{\ell t})^p - \eta \Delta x_{\ell t} - \Delta \varphi_t \right)^2, \quad (5)$$

where ζ , θ_p , η , and φ_t are parameters to be estimated. The above equations indicate that we have $\mathcal{P} + 1$ endogenous variables. The instruments are then the change in the scoring rule dummy and the change in the interaction of the scoring rule and the \mathcal{P} th polynomial of years after the investment.

Besides adjustment effects, we also pay attention to treatment heterogeneity by investigating whether the treatment is different for higher-ranked neighborhoods in terms of z -score to investigate whether place-based policies are more effective in more deprived neighborhoods and how the treatment varies with demographic characteristics. This is interesting because the effects may be different for high- and low-income households, in line with Diamond and McQuade (2016), or may be stronger in denser areas, where spillovers are more likely to arise because properties are closer to each other.

C. Data and Descriptives

Our analysis is based on a house transactions data set from the NVM (Dutch Association of Real Estate Agents). It contains information on about 80% of transactions between 2000 and 2014, so roughly seven years before and after the investment took place. For 1,796,542 transactions, we know the transaction price, list price, sales time (in days on the market), exact location, and a wide range of house attributes such as size (in square meters), type of house, number of rooms, and construction year. We exclude a few outlier observations.¹⁰ These selections do not influence the results. On average, properties in our sample are sold 1.29 times in our study period. In our main analysis, we focus on repeated sales, so properties that are sold at least twice, leave us with 434,033 transactions.¹¹

We report descriptives in table B2 in appendix B.1. It appears that about 3.8% of the observations in the repeated sales sample (16,726 observations) are in a KW neighborhood of which 42% of the transactions are from after the investment started. The price per square meter in KW neighborhoods is 3.5% lower than in non-KW neighborhoods. The difference is small but consistent with the observation that most deprived neighborhoods are located in urban rather than rural areas, where prices are generally higher.¹² Table B3 in appendix B.1 also reports descriptive statistics for the full sample, including properties that are transacted only once during the study period. It appears that there are few systematic differences between the full sample and the repeated sales sample.

In figure B2 in appendix B.1, we plot the house price for KW and other neighborhoods over time. It is confirmed that prices in KW-neighborhoods were lower than in other neighborhoods, but this price gap is substantially reduced after 2007, while from 2009 on, house prices seem almost identical. Although suggestive, one may not conclude that this reduction in price gap is due to the investment program, because it ignores that other factors may play a role (e.g., gentrification, disproportionate construction of new houses).¹³

¹⁰We exclude transactions with prices that are above €1.5 million or below €25,000 or a square meter price below €250 or above €5,000. Furthermore, we exclude transactions that refer to properties smaller than 25 or larger than 250 square meters. We drop a few properties that are sold more than five times in our study period or more than twice in one year or are listed for more than five years on the market or were listed zero days on the market. We also exclude observations for which the ratio of transaction to list price is below 0.7 or above 1.1.

¹¹Using repeated sales may imply a selection problem, because certain house types may be sold less often. In appendix C.10, we check whether our results are robust with respect to this selection.

¹²Properties in KW-neighborhoods tend to have a lower quality: they are more often apartments, are older, less often have central heating, and are of a lower maintenance quality. Also, 34% of the properties in these areas were constructed between 1961 and 1970, a building period that is associated with low building quality in the Netherlands.

¹³In appendix B.1, we show that the sales time for targeted and nontargeted neighborhoods is similar until 2007. After the investment, the sales time is much lower in KW-neighborhoods than in other neighborhoods. This difference seems to become somewhat smaller over time and disappears in 2013.

To allow for changes in neighborhood demographics and for treatment heterogeneity, we gather data from *Statistics Netherlands* on demographics, including average income, population density, the share of foreigners, age composition, and household size. For income, data are missing before 2004. Hence, we impute income data using national growth and 2004 income level. Our results are identical if we exclude years before 2004. We also obtain detailed land use data from *Statistics Netherlands* on the share of residential land, industrial land, land used for infrastructure, open space, and water bodies.¹⁴

D. Graphical Analysis

In figure 2A we plot price changes around the threshold while controlling for the z -score using a third-order polynomial. Note that our identification strategy is not based on a standard RDD design in levels. The latter would require stronger identifying assumptions because it requires that not only time-varying but also time-invariant unobservable factors should be uncorrelated to the treatment around the cut-off. Because we identify the effect based on changes, only time-varying unobservables should be uncorrelated to the treatment around the cutoff, whereas we allow time-invariant unobservables to be correlated. Moreover, because many (unobservable) factors that influence prices are omitted, the approach using variation in price levels may be less efficient and lead to larger standard errors than the approach using variation in price *changes* (Imbens & Lemieux, 2008). We therefore exploit variation in prices before and after the treatment and around the threshold. Price changes seem to be about 3% higher when a neighborhood exceeds the z -score threshold.¹⁵ The price difference is statistically significant at the 1% level. We will also focus on sales time to examine adjustment effects. In figure 2B, we show that sales times are statistically significantly lower (at the 5% level) when $z > 7.3$. This graphical analysis hides that the price and sales time effects might differ in the short and long runs, something we address in section V.

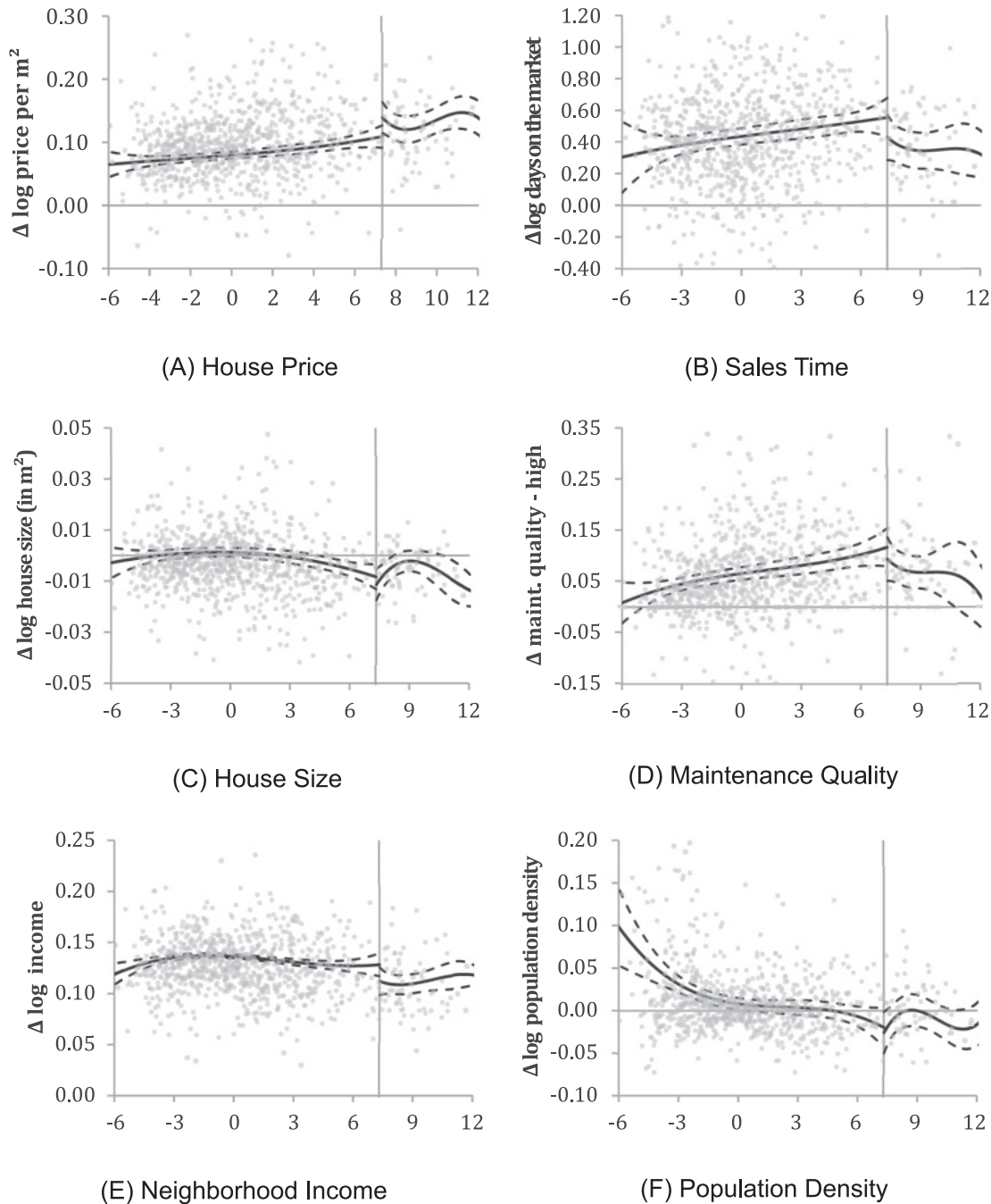
We also test whether changes in covariates are continuous at the threshold. In figures 2C and 2D, we look at differences in changes in house size and maintenance quality, respectively. If owner-occupied properties would be directly targeted by the place-based investment, one would expect a change in maintenance quality or house size. We do not observe significant changes confirming that home owners are only indirectly affected by the policy.

In figures 2E and 2F, we investigate changes in demographics. We emphasize here that those changes do not necessarily have to be continuous at the threshold, as changes in

¹⁴The land use data are available only for the years 2000, 2003, 2006, 2008, and 2010, so we impute land use for the intermediate years and assume that land use has not changed since 2010.

¹⁵These results (available on request) are essentially identical if we use higher-order polynomials.

FIGURE 2.—CHANGES IN COVARIATES AROUND THE THRESHOLD



These are weighted FRD estimates with a third-order polynomial on both sides of the threshold. The weights are equal to the inverse of the number of observations in a neighborhood. Each dot is a conditional average for a given z -score. We exclude observations within 2.5 km of a treated neighborhood.

demographics may be a direct result of the policy.¹⁶ It can be seen that neighborhood income is about 1.5% lower after the investment, which is statistically significant at the 5% level.

¹⁶Gerritsen, Webbink, and Ter Weel (2017) provide some evidence that the share of foreigners (in levels) may be discrete at the z -score threshold. Because we investigate the effect of a change in the treatment on changes in house prices, this is not a major problem. Nevertheless, we have also estimated ancillary regressions where we control for demographics in levels. The estimated coefficient is similar, albeit even somewhat stronger than the baseline estimate.

We do not know whether this reduction is due to changes in the composition of the public housing tenants—for example, because public housing associations accepted different tenants—or due to changes of households in nonpublic housing, but it implies that after a place-based investment, neighborhoods become slightly more attractive to poor households, in line with arguments of Diamond and McQuade (2016). For population density, we do not observe statistically significant changes. In appendix B.6, we investigate the effects on all

TABLE 2.—REGRESSION RESULTS: THE EFFECT OF PLACE-BASED POLICIES ON HOUSE PRICES

	Dependent Variable: Change in Log House Price per Square Meter					
	(1) OLS	(2) OLS	(3) SRD	(4) FRD	(5) FRD	(6) FRD
Δ KW-investment	0.0526*** (0.0123)	0.0452*** (0.0111)	0.0426*** (0.0119)	0.0412*** (0.0127)	0.0430*** (0.0125)	0.0332*** (0.0115)
Δ Housing characteristics (5)	No	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Neighborhood variables (10)	No	No	No	No	No	Yes
Number of observations	185,072	185,072	28,476	24,170	16,839	11,579
Number of clusters	3,138	3,138	257	176	285	184
R^2 -within	0.365	0.526	0.543			
Kleibergen-Paap F -statistic				4,797	9,191	2,592
Bandwidth h^*			4.312	3.229	4.600	3.419

We exclude observations within 2.5 kilometers of targeted areas. In column 3, we exclude nontargeted neighborhoods with a z -score above 7.3 and targeted neighborhood with a z -score below 7.3. In columns 4–6, the change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighborhood level and in parentheses. Significant at ***0.01, **0.05, *0.10, +0.15.

demographics in more detail using a local linear approach. We also test whether the KW-program has influenced homeownership shares. This may be important, as (part of) the treatment effect may be due to changes in homeownership rates through selling of public housing.

To investigate whether we measure an effect of sorting or whether the treatment effect captures changes in the neighborhoods' amenity levels, we estimate specifications where we control for demographic characteristics and homeownership shares and show that the treatment effects are essentially the same.¹⁷ Furthermore, we investigate treatment heterogeneity in more detail in section IV.C.

IV. Results

A. Baseline Results

We analyze the price effect in the neighborhood that received the KW-investment compared to the nontreated neighborhoods. Table 2 reports the regression results.¹⁸

We start with a naive regression of the change in house price on the change in the treatment status. The coefficient in column 1 shows that investments seem to have generated a positive effect on prices of $e^{0.0526} - 1 = 5.4\%$. When we control for changes in housing attributes (column 2), prices in targeted neighborhoods have increased by 4.6% relative to prices in other neighborhoods. In column 3, we employ a sharp regression-discontinuity design by excluding non-complying neighborhoods. We find an optimal bandwidth of 4.3, which implies that we include only about 15% of the observations. The price effect is 4.4% and similar to the previous specification.¹⁹ Because the neighborhoods that were

not treated while they have a sufficiently high z -score might be a nonrandom sample of the neighborhoods with $z \geq 7.3$, it is preferable to employ a fuzzy regression-discontinuity design. In the first stage, we regress the change in the assignment variable on the change in the scoring rule of a property (see table B5 in appendix B.4). In all the specifications, having a z -score above the threshold is a very strong instrument of being treated ($F > 2,500$), with a coefficient close to 1; houses that are in a neighborhood with $z > 7.3$ have an approximately 98% higher probability of becoming treated. The second-stage results are in line with previous specifications. The result in column 4, table 2, implies that prices in KW-neighborhoods have increased by 4.2% due to the investment program. In column 5, we explore the robustness of the findings further by removing the observations that are referring to transactions that occur before or after the treatment date. While this reduces the sample size by about 30%, this hardly has an impact on the price effect (4.4%).

Column 6 sheds some light on the potential mechanisms driving the price effect. Place-based policies may increase the amenity level but may also influence the composition of the population. For example, when the types of houses in the neighborhood increase due to the place-based policy, age composition of the households may change. These indirect effects may partly explain the effects on prices. In appendix B.5, we explore whether neighborhood demographics are influenced by the policy. We find evidence that KW-neighborhoods have seen a relative decrease in neighborhood income and an increase in the share of foreigners, as well as an decrease in the share of elderly people (over 65 years). Also, the average household size seems to have increased.

To test whether changes in the demographics induced by the program have caused the price changes or whether the effect of the place-based investments is mainly due to a direct change in the quality of nearby public housing, we control for additional demographic variables in column 6, table 2. More specifically, we include changes in population density,

¹⁷We address potential endogeneity of demographic variables in appendix C.2.

¹⁸In all specifications, we cluster the standard errors at the neighborhood level because the treatment varies at that level. One may argue that treated neighborhoods are often concentrated in space. We also have estimated standard errors while considering treated neighborhoods that are adjacent to each other as one. This makes little difference for the estimated standard errors. The results are available on request.

¹⁹One may argue that controls are not necessary in a valid RDD. The point estimates are essentially identical if we exclude control variables but slightly

less precise. Nevertheless, the estimates are always at least statistically significant at the 5% level. Those results are available on request.

TABLE 3.—REGRESSION RESULTS: ADJUSTMENT EFFECTS

	Δ Price per m ² (log)			Δ Days on the Market (log)		
	(1) FRD	(2) FRD	(3) FRD	(4) FRD	(5) FRD	(6) FRD
Δ KW-investment	0.0203** (0.00862)	0.0281** (0.0124)		−0.315*** (0.0682)	−0.326*** (0.104)	
Δ (KW-investment \times Years after investment)	0.00633*** (0.00222)	0.000307 (0.00458)		0.0366*** (0.0119)	0.0446 (0.0475)	
Δ (KW-investment \times Years after investment) ²		0.000764 (0.000707)			−0.00100 (0.00567)	
Δ KW-investment \times I(0.0–2.5 Years after investment)			0.0297*** (0.0102)			−0.281*** (0.0650)
Δ KW-investment \times I(2.5–5.0 Years after investment)			0.0416*** (0.0123)			−0.194*** (0.0576)
Δ KW-investment \times I(5.0–7.5 Years after investment)			0.0559*** (0.0190)			−0.0605 (0.0677)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	24,170	24,199	24,478	57,651	57,651	56,097
Number of clusters	176	177	178	545	545	6,144
Kleibergen-Paap <i>F</i> -statistic	2,413	1,334	2,108	5,252	3,370	22,206
Bandwidth h^*	3.242	3.257	3.287	6.219	6.211	6.144

The instruments are Δ scoring rule and the change in interactions of the scoring rule with the days after the investment. Standard errors are clustered at the neighborhood level. Significant at ***0.01, **0.05, *0.10, +0.15.

the share of foreigners, share of young (below 25 years) and elderly people and the average household size and land use. Increases in population density are associated with price increases. Furthermore, the share of foreigners is correlated with price decreases. More important, the coefficient of interest is hardly affected by including these controls (3.4%), which suggests that sorting on observable neighbor characteristics is not a main determinant of the effect of place-based policies.²⁰ This also seems to suggest that the effect of the place-based investments is mainly due to a direct change in the quality of nearby public housing rather than due to sorting effects.²¹

In what follows, we generalize our results in two directions. We investigate the steady-state dynamics and then pay attention to treatment heterogeneity.

B. Adjustment Effects

We now explicitly distinguish between short-run and long-run effects by allowing for adjustment effects. According to theory (see appendix A.3), we expect that the price effect is permanent. Sales times are expected to become smaller over time and disappear in the long run once the market has reached a new steady state. This is given the assumption that search costs are proportional to house prices—but only in the long run. To examine the latter, we also analyze the effects on log days on the market. We estimate equation (4) and use the

local linear approach without neighborhood variables, which corresponds to the specification listed in column 4 in table 2. We report the estimated coefficients in table 3.²²

In column 1, we include a linear interaction term of the treatment status with the time after the investment (measured in years). It is shown that there is an immediate price effect (2.0%). Also the interaction term is positive, so that the price effect becomes somewhat stronger over time. The specification predicts that after five years, the price effect is 5.1% (and statistically significant at the 1% level), which is similar to the baseline estimate. Column 2 also includes a second-order term leading to statistically insignificant interaction effects. However, it is more insightful to test the joint significance of these coefficients over time. The results are presented in figure C1 in appendix C.1. After five years, the price effect is 4.7%, while the immediate price effect is 2.8%. In column 3, we include interaction terms of the treatment variable and 2.5-year-interval dummies. The same pattern emerges: the price effect is increasing over time, but not so strongly (the *p*-value of 0.0393 indicates that the coefficients are significantly different at the 5% level). The price effect in the first 2.5 years might also be a bit lower because of uncertainty about the exact starting date of the program (an issue we discuss in more detail in appendix C.9). In any case, the results demonstrate that the price effect is permanent and that the price jumps once the policy was introduced.

We now investigate the adjustment effects of sales times after the announcement of the investment program. It seems that the sales time effect is immediate and substantial (see column 4 of table 3). The decrease in sales times is 27%, which is on average about a month's reduction in sales times. The

²⁰We explore this conclusion further in appendix C.2, where we instrument for potentially endogenous changes in neighborhood characteristics.

²¹It may be that due to the program, homeownership rates have changed so that part of the price effect may be attributed to an increase in homeownership. While we find some evidence in appendix B.6 that homeownership rates indeed have increased, we do not find evidence that the price effect is any different once we control for changes in homeownership rates (see appendix C.7).

²²The bandwidth is optimized assuming that the interaction terms are exogenous. Given that the bandwidth is similar for the SRD and the FRD, we do not expect that this has any impact on the results.

TABLE 4.—HETEROGENEITY IN THE ESTIMATED EFFECTS

	Δ Price per m ² (log)			Δ Days on the market (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD Rank	FRD Demographic Heterogeneity	SRD Investment Intensity	FRD Rank	FRD Demographic Heterogeneity	SRD Investment Intensity
Δ KW-investment	0.0375*** (0.0111)	0.0172 (0.0133)		−0.333*** (0.0973)	0.0144 (0.130)	
Δ (KW-investment \times Years after investment)				0.0420** (0.0207)	0.0630*** (0.0181)	
Δ (KW-investment \times Deprivation rank)	0.0143 (0.0138)			−0.0601 (0.0875)		
Δ (KW-investment \times Neighborhood income (<i>log</i>))		0.0341 (0.0404)			−0.594** (0.245)	
Δ (KW-investment \times Population density (<i>log</i>))		0.0622*** (0.0148)			−0.116+ (0.0725)	
Δ (KW-investment \times Share foreigners)		−0.0850 (0.0805)			−0.856* (0.455)	
Δ (KW-investment \times Share young people)		0.696** (0.347)			4.194 (3.920)	
Δ (KW-investment \times Share elderly people)		0.0829 (0.162)			3.063*** (0.979)	
Δ (KW-investment \times Average household size)		−0.127+ (0.0867)			−0.420 (0.603)	
Δ (KW-investment \times Investments per m ²)			0.00583 (0.00848)			−0.0195 (0.0488)
Δ (KW-investment \times (Investments per m ²) ²)			0.00204 (0.00158)			−0.0207* (0.0106)
Δ (KW-investment \times Years after investment \times Investments per m ²)						0.0137*** (0.00444)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Neighborhood characteristics (10)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	14,260	11,459	10,794	11,865	63,727	63,857
Number of clusters	241	179	168	192	2,285	2,295
R ² -within			0.532			0.056
Kleibergen-Paap <i>F</i> -statistic	703.8	169		387.9	283.8	
Bandwidth <i>h</i>	4.248	3.345	3.467	3.513	9.321	8.995

We exclude observations within 2.5 kilometers of targeted areas. The change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighborhood level. Significant at ***0.01, **0.05, *0.10, +0.15.

effect of sales times tends to become less pronounced over time. After five years, the effect is 12.3%. After 7.5 years, the effect is essentially 0 and highly insignificant. The same holds if we include a second-order term in column 5. Figure C1 in appendix C.1 shows the effects over time, displaying results that are very similar to the previous specification. Column 6 includes interaction terms, resembling the same pattern. The sales time effect is the strongest in the first period, while it converges to 0 within 7.5 years. Hence, these outcomes show that place-based investments have a permanent effect on house prices, while only a temporary effect on sales times because the market has to adjust to a new steady state. The results for sales time also give us more confidence in the results for house prices and provide us with information about the long-term price effect.²³

²³Recall that house prices and sales time tend to be negatively correlated. Let us suppose now that our house price results are completely spurious due to omitted variables. In that case, one would observe a permanent effect on sales time, in contrast to our results, which show a temporary effect on sales time.

C. Heterogeneity in the Treatment Effect

In this section we investigate whether we can detect heterogeneity in the treatment effect. We report results in table 4. As a first check, we interact the treatment effect with the deprivation rank. We normalize the rank to be between −1 and 1, with 0 being the average neighborhood, −1 being the worst-treated neighborhood, and 1 being the best-treated neighborhood (in terms of the *z*-score). The instrument is the change in the scoring rule and the rank if just the *z*-score was determined to select the neighborhoods. In column 1, we observe that the average treatment effect is 3.8%, which is close to the baseline estimate. The interaction effect is not statistically significant at conventional levels. For sales times, we observe a similar pattern (see column 5): the instantaneous effect does not seem to be stronger in worse-performing neighborhoods.

In column 2, we investigate whether the treatment effect is different for neighborhoods with different demographic characteristics. To this end, we interact the treatment effect with the same demographics as reported in table 2. To have a meaningful main effect, we subtract the locally weighted mean (based on the corresponding bandwidth) from the

demographic variable of interest. These results indicate that place-based investments are much more effective in dense areas. For example, when population density doubles, the treatment effect is 4.3 percentage points higher. We think this makes sense. Because we are interested in the external effect of investments in public housing on surrounding properties, it critically matters how close the properties are to each other. Our finding that the effects are stronger in denser areas suggests that there is a strong spatial decay in spillovers, in line with Rossi-Hansberg et al. (2010). There also seems to be a stronger effect in neighborhoods with a higher share of young people. However, this effect is counteracted by a negative effect of household size. If we exclude household size, the effect of young people is highly statistically insignificant. In column 5, we find somewhat different effects for sales times, although the results are not always statistically strong. The immediate effect on sales times seems to be lower in areas with lower income and that are denser. Furthermore, the sales time effect seems to be substantially less strong in areas with a high share of elderly people.²⁴

In column 3 of table 4, we use ancillary data on the direct spending by the national government per neighborhood (€250 million—about one-quarter of total spending). Because our instrument is not informative on the level of spending, we employ a sharp regression-discontinuity design where we exclude noncomplying neighborhoods. We then include the spending per square meter of the treated neighborhood area and the spending squared. The results suggest that the effect of spending is insignificant. However, when we plot the treatment effect as a function of spending (see figure C2 in appendix C.1), we find that neighborhoods that have received more funding per square meter also have a higher treatment effect. For example, the treatment effect for the average spending (€2.11 per m² neighborhood area) is 2.1%, similar to the baseline estimate, albeit a bit lower. For the neighborhood that received the most spending (€6.83 per m²) we find a treatment effect of 13.4%. We note, however, that the confidence intervals are not small. The effects on sales times are similar: it seems that for areas that have received more funding, the instantaneous sales time effect is stronger. We add one caveat to these results: we have only information on the direct spending by the national government, while the total spending intensity (including the spending by housing associations) may not be strongly correlated to the direct spending, as we do not know whether the spending by housing associations is positively or negatively related to the direct spending by the national government.

V. Sensitivity Analysis

We subject the baseline results to a wide range of additional robustness checks and ancillary regressions. In appendix C,

we discuss all sensitivity analyses in detail. Here, we restrict ourselves to a summary.

First, in appendix C.1, we investigate whether the baseline results reported in table 2 hold for time on the market, while including an interaction of sales time and years after the investments. The results indicate a consistent and strong negative effect of the place-based investment on sales time, which becomes less pronounced over time in all specifications.

In appendix C.2, we investigate whether the small decrease in the price effect when we control for changes in demographics and land use (see column 6, table 2) is mainly due to changes in neighborhood demographics or land use. We further instrument for potentially endogenous neighborhood characteristics and show that the price effect is very similar. This is followed in appendix C.3 by a sensitivity analysis for the heterogeneity in the treatment effect, leading to the same conclusion that place-based investments are more effective in denser areas.

Fourth, in appendix C.4 we investigate whether we can detect effects on the private and public housing rental market using information on housing surveys from 2002, 2006, 2009, 2012, and 2015. Each wave consists of about 60,000 respondents and is considered a representative survey of the Dutch population. The surveys provide information on a wide range of housing characteristics, including the rent paid and whether the property is rent controlled (always the case for the public housing stock). We first focus on the effects of the (noncontrolled) private rental stock. The preferred estimate seems to suggest that rents have increased by about 9% due to the KW-program. We do not find evidence of adjustment effects: the increase in private rents is immediate and permanent. A caveat here is that because the private rental market is such a small proportion of the housing market, the number of observations is rather low and the estimates are imprecise and therefore only marginally significant. We repeat those analyses for the regulated rent where we do not find any evidence of the KW-program having an effect on rents. Because those rents are rent controlled and essentially do not relate to underlying characteristics of the property or neighborhood, this result makes sense.

In appendix C.5, we run a set of quasi-placebo experiments. We investigate whether we can detect price changes in neighborhoods that were on a previous list of 340 neighborhoods that had some deprivation. Alternatively, we use a list of neighborhoods that receive investments by another program before the study period (so are in some way deprived) but do not receive KW-investments. A final placebo check involves the use of alternative deprivation scores to exploit the randomness in the determination of the *z*-score. We then investigate what happens if the 83 worst-performing neighborhoods would have been selected based on this alternative score. All of these quasi-placebo regressions support the conclusion that our results indeed are driven by the KW-investment and not by other investments or a general price trend in deprived neighborhoods.

²⁴In appendix C2, we test the robustness of these results by adding the interactions with the demographic variables one by one. This leads to similar conclusions.

Sixth, in appendix C.6, we further investigate the issue of unobserved trends that may be correlated to the treatment. In particular, one may argue that the year of implementation is about at the peak of house prices. Mean reversion would then imply that prices of KW- and non-KW-neighborhoods converge. As a check, we therefore exclude transactions around the peak of the housing market, between 2005 and 2011. Although this strongly reduces the number of observations, the coefficients are essentially unaffected despite the somewhat larger standard error. Although we employ an RDD and control for neighborhood income, one might still be worried that our results are driven by either city-specific price trends or by the more general trend that city centers seem to become more attractive. Because many treated neighborhoods are close to the historic city center, they may benefit from trends like gentrification that occur in and near the center. We continue by controlling for the distance to the nearest city center with at least 50,000 inhabitants. It appears that places closer to the city center have indeed become more expensive. The treatment effects, however, are essentially unaffected.

Seventh, we examine in appendix C.7 whether spatial spillovers of the investment program are important. When allowing for spatial spillovers, we need to take into account that several KW-neighborhoods are located close to each other, so that properties outside these neighborhoods benefit from spatial spillovers from multiple treated areas. Hence, we include the number of treated neighborhoods within 500 meter rings of the property. The main effects are very similar to the baseline estimates, while spillovers are largely statistically insignificant.

In appendix C.8, we investigate whether controlling for the share of home ownership and the share of private rental housing changes the results. This may be important for the interpretation of the result as the program led to some increases (though small ones) in the share of owner-occupied housing (see appendix B.6). However, using information on housing surveys for 2002, 2006, 2009, and 2012, we do not find any evidence that our results can be explained by changes in homeownership, as the treatment effect is essentially identical once we control for changes in the share of private rental and owner-occupied housing. Hence, it seems that the policy did indeed have an effect through the improvement of public housing.

Ninth, we test robustness of our results with respect to the starting date of the investment in appendix C.9. Although the official announcement of the program was on March 22, 2007, it was not clear when and how much money would be invested in the neighborhoods. As the starting date of the KW-scheme, we therefore use the date at which the secretary of state agreed with large public housing associations that they would invest in the KW-neighborhoods. However, the uncertainty on the exact starting date of the program seems not to matter for our results.

Tenth, in appendix C.10, we test for the robustness of our results to assumptions with respect to the bandwidth of the local linear regression approach. This is followed by an inves-

tigation of whether using the full sample, rather than repeated sales influences our results. The coefficients are very similar to the baseline specification—if anything, they are slightly higher. The results are reported in appendix C.11.

Finally, we confirm in appendix C.12 that our results are robust to the choice of identification strategy by employing a nonparametric propensity score matching method rather than a regression-discontinuity approach.

VI. The KW-Program and the Overall Gains in Property Values

We seek to gain insight into the rate of return through the external effect of the revitalization policy. We reiterate that we measure external effects because we focus on investments in the public housing stock on the prices of owner-occupied properties. Hence, we do not have estimates on the direct effect of the program. Nevertheless, the direct effects are expected to be positive. Because we focus on the external effects, the calculated rate of return will serve as a strong underestimate of the total benefits of the program.²⁵

We use additional data on the number of housing units from Statistics Netherlands. We estimate the benefits and costs in 2007 prices by deflating house prices by the consumer price index, obtained from Statistics Netherlands. We assume that the average price is constant across the study period, so $p_{\ell t} = p_{\ell}$. To estimate the average price for owner-occupied housing in each neighborhood, we take the average of deflated prices of all transactions in our study period. Furthermore, we gather data on the average house prices of all properties in each neighborhood, including rental properties, which have slightly lower housing values than owner-occupied properties.²⁶ Table 5 reports the back-of-the-envelope calculations for different scenarios.²⁷ We start with the parsimonious estimate of the benefits. The average increase in house prices is then about €5,000, which is approximately 3% of the mean house price. The effect is somewhat higher once we use the long-run estimate. The total benefits for homeowners are about €0.5 billion. The results indicate the gain-to-funding ratio is about 0.5 given the realized investments of €1 billion.

To also include the benefits on (noncontrolled) private renters, we use the average house prices of all properties. In the second calculation, we assume that the effect on private

²⁵Expenditures through the KW-program were financed from additional and external sources and were not part of the municipal budget or the budget of housing associations. In contrast, when expenditures are raised by limiting expenses in other neighborhoods, for example, this may imply that positive externalities are reduced in nontargeted areas (Rossi-Hansberg et al., 2010).

²⁶We ignore that house owners can deduct their interest mortgage payments from their income, so prices of owner-occupied housing may somewhat exceed housing values compared to an unregulated market.

²⁷One may argue that the welfare calculation is incomplete because we do not take into account the welfare benefits that arise in neighborhoods that are close but did not get the subsidy (Glaeser & Gottlieb, 2008). We showed weak evidence for spatial spillovers in the sensitivity analysis, although the confidence intervals are quite large. Hence, the estimates presented here are, if anything, underestimates of the total effects of place-based policies.

TABLE 5.—TOTAL BENEFITS OF THE KW-PROGRAM

Scenarios	Benefits per Property Owner (in €)		Total Benefits (in billion €)	
	Baseline Estimate	Long-Run Estimate	Baseline Estimate	Long-Run Estimate
Owner-occupied housing	5,224	6,347	0.481	0.585
Owner-occupied and private-rental housing: Effect on private-rental equal to effect on owner-occupied housing	5,200	6,318	0.824	1.001
Owner-occupied and private-rental housing	9,715	12,258	1.539	1.942
All housing: Effect on public housing equal to effect on private-rental housing	13,102	16,723	4.951	6.319

The estimated benefits are in 2007 prices. Information on number of housing units is based on Statistics Netherlands 2012.

renters is the same as for homeowners. Because the share of owner-occupied housing is small in KW-neighborhoods (only 24%), the benefits are now substantially larger: the gain-to-funding ratio is about 0.8.

It may be preferable to explicitly use the point estimates for the private rental market as shown and discussed in the sensitivity analysis. Because we find stronger effects for private rents than for the owner-occupied housing market (see appendix C.4), the average benefits to property owners (including landlords) are 85% higher. This also implies gain-to-funding ratios that are above 1, so the external effects of the KW-investment program are larger than the supposed investments.

Homeowners living in the neighborhood at the time of the investments have benefited directly from the program due to the increase in house prices. Homeowners are usually higher-income households. The KW-program was meant to help poor households and reduce inequality, so homeowners were not the intended beneficiaries of the program. We have shown that the subsidy does not capitalize in controlled rents (see appendix C.4), so public housing renters enjoy the positive neighborhood effects that are caused by the program without paying for it. To get a rough idea of the total benefits to renters in public housing, we assume an identical monetary effect on public rental housing as on private rental housing. The average benefits per property are then about €13,000. This also implies that the gain-to-funding ratios are substantially larger and around 5, so that about two-thirds of the external benefits of the program accrue to renters in public housing.²⁸ Note that public housing renters, besides the increase in neighborhood quality, also have benefited directly from improvements in their properties. In other words, the program seems to have been effective in increasing the welfare of the poor.

The latter estimates probably serve as an upper bound of the external benefits of the program, but we note that the estimates are still in line with Rossi-Hansberg et al. (2010), who also found substantial gain-to-funding ratios for an urban renewal project in Richmond, Virginia.

²⁸The total benefits in the fourth scenario are €4.95 billion; they are €1.54 billion when we ignore public housing tenants. Hence, 69% of the total benefits accrue to renters in the public housing sector. We note that this estimate is given the assumption that the effect on public housing is equal to effect on private-rental housing, as measured in appendix C.4.

VII. Conclusion

In many countries, governments invest in deprived neighborhoods to reduce income disparities within cities and fight social problems. In Europe, this mainly involves an improvement in the quality of the public housing stock. There is a limited understanding to what extent such a place-based policy is effective and has external positive effects on nearby residents.

In this paper, we aim to estimate the external effects on nearby households in the owner-occupied market of a nationwide investment program that improved the quality of public housing in the 84 most deprived neighborhoods in the Netherlands. A rich repeated sales data set on house sales in the period 2000 to 2014 is used. We explicitly take into account that treated neighborhoods are not randomly chosen by governments. We combine a first-differences approach with a (fuzzy) regression-discontinuity design based on a jump in the probability of being treated, which depends on neighborhood-specific deprivation scores. We find compelling evidence for the presence of positive external effects on nearby property owners of the investment scheme. The program has led to an increase in surrounding house prices of 3.5% and to temporary reductions in sales time that disappear after 7.5 years. We also find evidence for treatment heterogeneity: the effect is stronger in dense areas, likely because spillovers are more pronounced when properties are closer to each other. We calculated that the welfare benefits to property owners induced by the place-based policy program are sizable and at least half of the value of the expenditure on public housing. Moreover, public housing renters benefit from improvements in their properties and neighborhood quality, while not paying a higher rent and are therefore better off. Hence, the program has been effective in increasing the welfare of poor households.

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